unknown referee as any evidence of these could then be referred back to the referee's institution. The bench test of a piece of research is its shelf life: will it still be of interest or relevant in 10 years' time? This test could equally well apply to negative findings. Many important discoveries throughout history have initially been considered to be unacceptable on religious or cultural grounds. As the ultimate peer reviewer is the wider medical audience, the medical referee should perhaps just act as the referee without acting simultaneously as the goalkeeper, leaving readers and subsequent events to be the best judge of a paper's worth.

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1 Smith R. Promoting research into peer review. *BMJ* 1994;309: 143-4. (16 July.)

Referees should provide references

EDITOR,—I applaud the *BMJ*'s efforts to improve the quality of the peer review process.[1] A small improvement would come if referees were under the same obligation as authors to provide references in support of statements drawn from the literature. A critical review should be a catalyst to renewed and improved effort. This is not the effect if an author is unable to trace the source of a reviewer's criticism; instead, the temptation is to question the motives or abilities of the reviewer and even to question the probity of the journal he or she represents. This should also apply to reviews of applications for research grants, when a vague, unsupported statement saying "something like this has been done before" will result in almost certain rejection.

If a reviewer is genuinely in a position to give an expert opinion the addition of references should mean little extra work. If a bibliographic database is used[2] there is even no need for extra typing.

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1 Smith R. Promoting research into peer review. *BMJ* 1994;309: 143-4. (16 July.)
2 Jones RG. Personal computer software for handling references from CD-ROM and mainframe sources for scientific and medical reports. *BMJ* 1993;307:180-4.

The role of letters in reviewing research

EDITOR,—R S Bhopal and Alison Tonks have pointed out that the potential of material on the correspondence pages remains underdeveloped and undervalued.[1] An editorial by Charlton published in *Anaesthesia* included some letters to the editor without making any reference to those letters or their authors.[2] Charlton had been advised by the journal's editor that "it would not be normal to refer to the correspondence when referencing an editorial about a topic of major interest" (personal communication.) It is perplexing that recommendations published in correspondence can be used but their authors not credited. It is high time for the editors of the leading international scientific journals to formulate guidance for authors of articles and editorials so that they cite published correspondence when they use the message contained in such letters.

Letters to the editor that are in the form of suggestions or recommendations that do not necessarily comment on a published article should also be considered. Bhopal and Tonks also stated that if literature searches of published reports are to include relevant letters, corrections, and other comments, then a system will need to be developed systematically and reliably to link papers with other relevant material; indexing of all letters to original research must be the first step. In my opinion such indexing should not be restricted to letters responding to original research: it should include other letters of wide interest. Implementing the suggestions put forth in such letters to the editor might be beyond the scope of an individual, demanding participation and commitment of institutions. Some authors might not be backed by fully equipped laboratories and other infrastructure to scientifically validate their ideas. Letters to the editor are one outlet for airing those ideas. These need encouragement, not reproach.

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1 Bhopal RS, Tonks A. The role of letters in reviewing research. *BMJ* 1994;308:1582-3. (18 June.)
2 Charlton JE. Checklists and patient safety. *Anaesthesia* 1990;45: 425-6.

Statistics notes

Defining sensitivity and specificity

EDITOR,—In their statistics note on the sensitivity and specificity of diagnostic tests Douglas G Altman and J Martin Bland's idiosyncratic use of the term "true positive" is unconventional and unhelpful and will confuse readers.[1] Since 1947, when Yerushalmy introduced the terms sensitivity and specificity to aid understanding of the utility of diagnostic tests,[2] respected authorities on both sides of the Atlantic have used the term true positives to indicate those cases in which the disease is present and the diagnostic test gives a positive result.[3-5] To use the term to mean all cases of the disease regardless of the test result is a redundancy, and to use it in this way when defining sensitivity and specificity further obfuscates what for several students and physicians (and perhaps statisticians?) is already confusing.

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1 Altman DG, Bland JM. Diagnostic tests 1: sensitivity and specificity. *BMJ* 1994;308:1552. (11 June.)
2 Yerushalmy J. Statistical problems in assessing methods of medical diagnosis, with special reference to x-ray techniques. *Public Health Rep* 1947;62:1432-49.
3 Rose G, Barker DJP. *Epidemiology for the uninitiated.* London: BMA, 1979.
4 Sackett DL, Haynes RB, Guyatt GH, Tugwell P. *Clinical epidemiology: a basic science for clinical medicine.* Toronto: Little, Brown, 1991.
5 Armitage P. *Statistical methods in medical research.* New York: Wiley, 1977:433.

Regression towards the mean

EDITOR,—J Martin Bland and Douglas G Altman's note on regression towards the mean fails to convey the process underlying the phenomenon except in mathematical terms.[1] I hope that the following account may make the relation between regression towards the mean and the correlation coefficient r more understandable.

Assume that there is perfect correlation between a child's height and the mid-parental height ($r=1$). Then one can predict, without error, the child's height from knowledge of his or her parents' height and vice versa. If the mid-parental height is above the mean then so will the child's be, by an equal amount. There is no regression to the mean.

On the other hand, assume that a person's height is completely unpredictable from knowledge of his or her parents' height. There is no correlation between parental and child height ($r=0$). Tall parents will have had this unpredictable effect act in the direction of making them tall, but their children will have an equal likelihood of being tall or small. The mean height of the children of tall parents will be the mean height of all children. There is complete regression towards the mean.

In reality r lies between 1 and 0; there is a predictable component relating parental height to child height and, in addition, an unpredictable component. If a child's parents are tall then the likelihood is that the chance effect acted to increase the parents' heights above what would have been predicted. These chance effects are not heritable (by definition), and the child's height, on average, is therefore closer to the mean height of all children than the parents' heights are to the mean height of all parents. The larger the unpredictable component relative to the predictable component the smaller is r and the more likely it is that a person's height deviates from the mean because of chance effects. These unpredictable effects will not be seen in the relative whose height is closer to the mean. Therefore the smaller is r the greater is the regression to the mean.

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1 Bland MJ, Altman DG. Regression towards the mean. *BMJ* 1994;308:1499. (4 June.)

Authors' reply

EDITOR,—We agree with Gordon L Dickie that in the context of diagnostic tests the term "true positives" is usually used to mean those people with the disease in whom the diagnostic test gives a positive result. We think that our meaning was clear and hope that not too many readers were dismayed by our non-standard terminology.

Regression towards the mean is a difficult concept. We hope that *BMJ* readers will find Simon Fleminger's description helpful.

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Patients are unwilling to enter clinical trials

EDITOR,—T C B Dehn is right to draw attention to the problems of obtaining informed consent from patients entering randomised trials, particularly when one option entails additional, potentially toxic treatment, such as chemotherapy.[1] But Dehn's letter also illustrates the problem of avoiding bias when describing a trial: Dehn admits that five of six patients refused to enter a trial comparing preoperative chemotherapy with surgery alone for oesophageal cancer because of anxiety that they might get chemotherapy. I wonder if, had the patients been seen by an oncologist, a similar number might not have refused because they did not want to miss out on the "beneficial" effects of the chemotherapy.

A colleague and I are both committed to a particular trial testing the value of prophylactic cranial irradiation in small cell lung cancer. We both think that we are honest in our description of the hazards and benefits, and, while many of my patients refuse because they do not want the risk of toxicity, many of hers refuse because they do not